LETTERS TO THE EDITOR.

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The Perception of Sound.

PROF. KEITH has replied to the physiological criticisms in Prof. Bayliss's most thoughtful letter in NATURE of October 17. I will therefore confine myself principally to the physical objections he has raised.

With regard to the difference between the molecular movement of a liquid subjected to the pressure of a sound-wave, and the molar motion of a minute mass of liquid in the cochlea, it cannot be forgotten that liquids could not conduct sound "unless they were both elastic and compressible."

The fact that water is so difficult to compress, and this only at very high unit pressures, is proof of its high degree of elasticity or tendency for its molecules to return to their undisturbed positions. But an extremely low unit pressure will cause molar motion in a small mass of liquid moving in a vessel of suitable shape. It is to such molar motion that I refer. A reference to my footnote (p. 56 of my book) shows that Helmholtz in his later studies recognised that "an incompressible fluid contained between solid walls is distinguishable from a compressible one in this: that every impulse which reaches any part of its surface communicates itself immediately throughout the whole fluid and sets every part instantly in motion, while in a compressible fluid a wave proceeds from the spot of action and travels with a certain velocity, setting in motion the several particles of the fluid consecutively. Thus if the dimensions of the whole mass are infinitely small in comparison with the wave-length (as it is in the labyrinth water), and if the walls of the petrous bone which enclose it are so solid that we can consider them as absolutely so when compared with the minute pressure under consideration, we deduce the following: The communication of action throughout the whole mass is practically instantaneous, and the labyrinth water under the influence of the sound waves moves virtually as a fluid absolutely incompressible (and therefore incapable of sound waves) would move under the same relations."

I am glad that Prof. Bayliss directs attention to the difficulty he finds in seeing how there is a difference of pressure on the upper and lower surfaces of the basilar membrane when the column is a continuous one.

The answer is that the high pressure on the upper side of the membrane is carried through the bent levers of the Corti arches to do mechanical work in bending the hairlets (see p. 138). In science it is true, as in more homely affairs, that we cannot both eat our cake and have it. As the pressure is passed out on the upper side of the membrane to do work in the scala media in bending thousands of hairlets, there is an equivalent loss of pressure in the passage below the membrane. In a double-acting hydraulic engine we have a somewhat similar condition of things. The high-pressure water is admitted into a cylinder and the piston is pushed forward and backward alternately. The piston-rod carries the pressure to the crank-shaft, thus doing external work. The equivalent unit pressure disappears in the exhaust water which is pushed out at atmospheric pressure. The same displacement of liquid takes place in the exhaust, but the displacement is reduced in pressure by the equivalent amount of work carried out of the system.

Prof. Bayliss asks why there are so many Corti arches when a few might suffice. This I take to be necessary in order to extend the surface of excitation, which I have endeavoured to describe on p. 72; also to provide for the probability that during a lifetime of continuous work many of the Corti arches would fail to function.

With regard to Yoshii's experiments, I have not seen them, but Prof. Keith has informed me that such observations have been made, though he did not describe the conditions under which the experiments had been carried out If the observations depended upon prolonged exposure to the vibrations of a musical note, I scarcely think they should be regarded as a proof of the localisation of certain strings in the basilar membrane to produce certain notes. It might only mean that some weak part in the system gave way and ceased to function under a stress of fatigue. It seems quite possible, as suggested by Prof. Keith, that notes of high frequency, rising very rapidly to their maximum pressure, might tend to short-circuit towards the narrow end of the membrane, and notes of low frequency, rising slowly to their maximum, might impress the wider end of the membrane, but it must be remembered that the areas at the fenestral end are extremely small, and that Helmholtz himself recognised the practically instantaneous action of the pressure throughout the passage.

The strongest argument against the string instrument theory is that in the basilar membrane all the so-called strings are cemented together sideways, and cannot, therefore, vibrate freely or respond each to its own vibration rate.

In the displacement theory the striate or inelastic portion of the membrane moves downwards as a whole, following the same spiral plane on a hinge coinciding with the tapered edge of the aperture, each strip of its breadth descending a distance increasing from the fenestral end to a maximum at the helicotremal end, and in this way obliging the triangular displacements to increase towards that end uniformly.

In a compound-wave form entering the ear we have indications not only of the original impulses of the pure tones from which the compound is built up, but also of the differential tones, the summational tones, and harmonics.

When we find these all reappearing as sensations in the brain, the conclusion appears to be forced upon us that the ear must be a machine adapted to sustain on their passage to the brain these impulses of which we find indications in the wave-form.

My endeavour has been to examine the structure and working of this marvellous machine which causes the accurate transference of such impulses.

THOMAS WRIGHTSON.

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THE objections against Sir Thomas Wrightson's theory raised by Prof. Bayliss in NATURE of October 17, though grave, no doubt, to those who can appreciate their cogency, yet appear to one who has attempted to approach the problem from the side of phonetics comparatively slight; while there are objections to the prevalent theory which, if I may be allowed briefly to state and develop them, may perhaps throw some light on the question at large.

I would put the matter this way. The human ear as imagined by Helmholtz is no great improvement on Nature, for it could neither (1) hear a note of music, nor (2) hear a large percentage of the words in the English language.

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(1) Advancing on the lines laid down by Sir Charles Bell and J. Müller, "that, *however excited*, each nerve of special sense gives rise to its own peculiar sensation" (Bayliss, "Principles of General Physiology," 1915, p. 513), Helmholtz put forward the hypothesis that each fibre of the auditory nerve "hears in its own peculiar pitch" ("Sensations of Tone," 1885, pp. 148 and 151) without regard, as it seems, to the immediate consequences which it must have for his main theory. For, from the rate of damping determined by him, "it follows that the natural pitch of the internal vibrators, which respond sensibly to a given simple sound, ranges over about a whole tone" (Rayleigh, "Theory of Sound," vol. ii., § 389). That means that when a note is sounded a great number of nerve-fibres are stimulated, for the Helmholtz mechanism of the internal ear requires every string of its remarkable pianoforte to be connected "with a nervous fibre in such a way that this fibre would be excited and experience a sensation every time the string vibrated" (p. 129). The number of strings or internal vibrators allotted to the interval of a whole tone varies. In 1870 it was 66⅔; in 1877 it was 100; but the number, if more than one, is immaterial here. Assuming that 100 strings of the basilar membrane vibrate in unison with the given note, 99 of these will be executing forced vibrations at other than their proper frequencies, and *ex hypothesi* 99 nerve-fibres will call up 99 dissonant sensations of tone, all of different pitch and of intensities diminishing regularly on either side of a maximum, which is due to the 100th fibre, the peculiar pitch of which agrees with the exciting note. As each nerve-fibre, *however excited*, gives rise to the sensation of its own peculiar pitch, it matters not whether the internal vibrators vibrate with their proper frequencies or with that of the imposed tone. Unless, therefore, there is in the central organ some contrivance, which Helmholtz does not provide, for inhibiting the odd 99 nerve-fibres, or a transformer of some kind to standardise their pitch, it follows that when a tuning-fork is made to vibrate, no note can be heard, but only an unimaginable din. Music would then be impossible; we could never hear anything but noise.

On the other hand, if Helmholtz had allowed each nerve-fibre to communicate the actual pitch of the vibrator connected with it, whether executing a free or a forced vibration, then there could never be a clean-cut, staccato ending to a note, but after a bass note has ceased externally to the ear there would ensue for about one-tenth of a second, according to his estimate—to the ear an appreciable period of time—a similar confused noise of many mistuned strings; for, by p. 144, "an elastic body set into sympathetic vibration by any tone vibrates sympathetically in the pitch number of the exciting tone; but as soon as the exciting tone ceases, it goes on sounding in the pitch number of its own proper tone." The cochlea (limaçon, Schnecke) well deserves its name. For, however many fibres its house may hold, the snail certainly has two horns. Sir Thomas Wrightson's theory presents us with no such dilemma as this.

(2) In 1916 I found that if I sing to a bass note such a vowel as *oh* or *oo*, and end the note staccato by closing the glottis (the "Glasgow" substitute in speech for occlusive *t* or *k*) while keeping the shape of the mouth unaltered, I hear that the harmonic of the voice which is reinforced by that excellent resonator, the cavity of the mouth, is still audible for a very brief space after the voice has ceased to be heard. From which I infer that the rate of damping in the internal ear is more rapid than that of the body of air in the mouth shaped for certain vowels. But as I do not expect or desire that this inference should be accepted as scientific fact merely on my statement, just as little am I disposed to accept Helmholtz's guesswork as an adequate basis for the calculation which was to have such far-reaching and subversive consequences, overthrowing, for example, the belief of Lagrange and Thomas Young that rapid beats may combine into a sensation of tone. Hence my previous letter (NATURE, May 16, 1918) with a kymograph tracing of the word "utter" intoned at pitch 100 and measured by a tuning-fork of the same pitch. In that tracing it is not a question of a note being reduced to one-tenth of its intensity in the time occupied by 9·5 vibrations, but well within that limit a loud note is reduced to silence. There is the proof that the unchecked estimate which is the very keystone of the Helmholtz theory of audition is wide of the mark.

The complete cessation of sound in "utter" is an essential feature of English and of other languages. It is astonishing that Ellis, the phonetician, never thought of this when translating Helmholtz. That which is *common* to the first *p*, *t*, or *k* in "stop, please," "or not to be," "bookcase," by virtue of which these three "sounds" are classed together as voiceless occlusives, is evidently a shock sensation of the sudden cessation of a sound. How the existence of such a sensation is to be reconciled with any resonance theory of audition has long been a puzzle to me. The very term *resonance* seems out of place in the presence of this phenomenon; and when, on May 17, Sir Thomas Wrightson's book came into my hands, the expression "*dead beat*" in his preface appeared to promise an advance towards the solution of a most complicated problem. W. PERRETT.

University College, London, October 23.

The Society of Civil Servants.

APROPOS of the letter which appeared in NATURE of October 24 on the need for scientific workers to organise themselves, I shall be obliged if you will allow me through your columns to direct the attention of scientific workers in the Government service to the recently founded body, the Society of Civil Servants, which is intended to cover the middle and upper grades of the Service—grades which hitherto have been almost wholly unorganised. By its second rule the objects of the society are defined as "to deal with all matters affecting the Civil Service, and to take such action thereon as may be expedient"—a purview of unlimited range. While the society is constituted on the basis of individual membership, members are encouraged to coalesce into whatever sectional associations—called in the rules "grade groups"—may conveniently and naturally come about. It is these "grade groups" that will consider matters such as salaries and scales of promotion which affect their members solely, the society taking up only wide questions affecting the Civil Service generally.

It is an old saying that "Providence helps those who help themselves." Scientific workers have in the past had just cause to complain of the niggardly treatment that they have experienced at the hands of the State. By organising themselves into "grade groups" of the society, according to the various Departments, those in the State employ will have an opportunity of directing attention to their claim for more generous treatment; but should they fail to take advantage of the present opportunity, they will have no one to blame but themselves if in the future they continue to receive the same neglect as in the past. It is no secret that a scheme for the application of the principles of the Whitley report to the

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